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MODERN ELECTRODYNAMICS.

Électricité et Optique: la Lumière et ses Théories Électrodynamiques; leçons professées à la Sorbonne en 1888, 1890 *et* 1899. Par H. Poincaré. Deuxième édition, revue et complétée par J. Blondin et E. Néculcéa. Pp. x+642. (Paris: G. Carré et C. Naud, 1901.) Price Fr. 22.

IN the present state of electrical and general physical theory there are probably few undertakings more useful towards progress than a critical discussion of the views of other writers by one who has himself thought deeply and read widely on the subject.

We may recall the stimulus afforded to the progress of Maxwell's electric theory on the Continent by Helmholtz's early series of critical memoirs (now largely out of date, having served their purpose) that were devoted to the examination of the relation in which that theory stood to the views of electrical action then current.

The lectures of M. Poincaré, reported and published by his pupils about ten years ago, possessed great interest as being an account of the then fresh advances constituted by the experimental investigations of Hertz, from the hand of a writer who occupied one of the highest positions both in the domain of pure mathematics and in that of its physical applications. The writer's unlimited command of analysis and the range of his interests were certain to shed new lights on the subject-matter of which he undertook the exposition. A second edition of the "Électricité et Optique" is now published in a volume of 640 pages, of which about half consists of a report of lectures delivered at the Sorbonne in 1899 on the still more recent improvements of Maxwell's electrodynamic theory which are associated largely with the name of H. A. Lorentz. It is this latter half of the book, giving the writer's reflections and criticisms on a development which is still fresh, that will naturally present the chief interest for others who have meantime been following the progress of the subject.

The main feature of the new standpoint is the resuscitation of the idea of electricity as representing something permanent like matter. In Maxwell's later writings, in which he was mainly occupied in eliminating the hypothetical illustrations and models which had guided him to his theory, but were not logically necessary to its formal exposition, there was a tendency for the older idea of an electric charge, as representing something real, to be eliminated. According to his view, the electric current always flows in closed circuits like a current of an incompressible fluid, so that there is nowhere any tendency to accumulation of electrodynamically effective electricity. It seemed, therefore, possible to do without any introduction in detail of an entity whose flow was restricted by the condition that the quantity of it in any given volume could never alter. This conception of circuital electric flow (to use Lord Kelvin's term) required the ascription of properties the same as those of currents to electric excitation both in dielectric material substances and in the free æther itself. The displacement current thus introduced is, in fact, the fundamental feature of Maxwell's electrodynamics. Its assumption led directly to a simple and perfectly complete theoretical account of the electrodynamics of material systems at rest, on the basis of laws established long before by Ampère and Faraday; the application to bodies in motion was, however, left by Maxwell in an incomplete and tentative state. In 1872, when he published his treatise, the circumstance that the laws of electrolysis imposed the idea that electricity was in some sense or other atomic was definitely realised, but with a certain reluctance; while in the treatment of bodies in motion the explicit recognition that a moving charge acts as a current had, owing to an oversight arising from his preoccupation with the medium, to be formally introduced into his equations by Fitzgerald ten years later, though Maxwell fully accepted such action as a fact all the time.

This plan of ignoring electricity and treating electrodynamics on the basis of a uniform medium with physical constants affected by the presence of matter, and subjected to various vector disturbances whose nature is unknown, but which are connected by partial differential relations expressing the laws of Ampère and Faraday, has been very fully developed by Heaviside and by Hertz. In both cases compensation is sought for the variation of the energy of each element of the medium, solely in the work of tractions exerted on its surface by the surrounding parts. In Heaviside's discussion the problem was treated with great generality and comprehensiveness; it will suffice here to pass in review the salient features of the more concise analysis advanced by Hertz. The treatment of stress by the method of energy requires displacement of the medium; and so the problem of ponderomotive forces becomes related to the general question of moving media, which is the part of the subject that provides the crucial tests of theory. The electromotive phenomena in media at rest are, on the other hand, all involved in the adoption of the aforesaid laws of Ampère and Faraday, as a description of the properties and behaviour of the medium. The same description is extended to media in motion after the manner of Faraday, and the deductive part of the argument is there confined to the determination of the ponderomotive mechanical forces. To obtain them, Hertz subjects his single uniform medium, which he takes to be the seat of the electric and magnetic energy, to static strain without finite motion, and computes the time-rate of alteration of the energies thereby produced in a given element of its mass. He supposes that the polarisations per unit volume, being affections of the medium, are simply convected along with it. If the element of mass were dynamically self-contained and not subject to tractions from the surrounding parts, its energy would be conserved so that this time-rate of alteration should vanish. As it is, the alteration does not vanish, but represents the work done in the element by the tractions acting on its surface. As the element is part of an elastic medium, the work of such surface-tractions ought to be expressible in the form of work of the stress-system, existing in the element, to which these tractions belong. Now the expression for the variation of the energy is thrown by Hertz into the latter form, in fact without the use of any electrodynamics in the analysis; and this leads him at once, for the case of isotropic media, to a self-conjugate electromagnetic stress-system, the same as Maxwell's, as

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providing the reacting mechanical forces required for equilibration of the outstanding energy. But for æolotropic media there arises a bodily torque in addition to this stress; this torque is included in Maxwell's general type of magnetic stress, and prevents it from being self-conjugate for that case. Hertz is unwilling to admit such a type of stress, which could not exist in an ordinary elastic solid; but he is at a loss to know what to do with this new part, and simply drops it, retaining the self-conjugate part as the stress in his electric medium. But for this the theory would be a consistent one on his premisses; and the result for the free æther, or wherever there is no material polarisation, is, in fact, the stress which Maxwell showed was competent to represent the actual mechanical forces. It is to be observed that this is all that is to be got out of the statical application of the principle of energy to his medium; the kinetics of the electromotive play being assumed as known, outstanding variations of the energy in slow changes are to be ascribed to the work of mechanical forces. No success has been achieved in the problem of reducing the electromotive play in media in motion to definite self-contained dynamics on any other basis than the theory of electrons; the charge must consist of discrete independent elements, each with its own electric field. The mode of treatment here sketched introduces, among other things, a mechanical force of an electric field on changing magnetic polarisation as the counterpart of the known mechanical force of a magnetic field on changing electric polarisation; this, on the theory of electrons, is non-existent.

The treatment of electrodynamics on the basis of discrete electrons is a branch of statistical molecular theory, like the kinetic theory of gases, and involves the refined considerations connected therewith, including the estimation of averages instead of the following out of individuals. The care that is thus necessary in the analysis may be illustrated by a temporary slip that has crept in at the beginning of the discussion of Lorentz's theory (§ 333), in which the single principle of continuity of flux of true electricity appears as a consequence of the addition of two independent formulæ, (3) and (4), neither of which appears again. They cannot be both true, or there would be two such principles of continuity instead of one. It would seem that the term density has been inadvertently used in two different senses, ultimately as the volume density of electric charge in the medium which depends on how closely the electrons are packed in it, but meanwhile as the density of electricity in an electron, supposed to be itself a small and rigid though mobile volume-distribution of charge. This local oversight, doubtless due to imperfect reporting of the lectures, illustrates an actual disadvantage of a completed hypothesis, which insists on a full specification of an electron, over the less complete physical specification, which, recognising that there is more in the constitution of the molecule of matter than our philosophy may ever reach, is content to regard it simply as the unknown central point or pole of its surrounding field of force.

The general plan of development of electrodynamics on this basis that is adopted by M. Poincaré consists in writing down equations of motion for each electron, by assigning to it a mass and considering it to be acted on by the averaged or smoothed-out electric and magnetic forces of the field that surrounds it, and finally passing to equations for the medium in bulk by summing or averaging the results for all the electrons per unit volume. This method is in keeping with the astronomical traditions of mathematical physics, in which the problem is put in definite terms at the beginning, and the analysis is confined to surmounting the difficulties, purely mathematical, that arise in its unravelment. There is, however, a different kind of theoretical physics which has had more success in this country, which recalls the names of Young, Stokes, Kelvin and Maxwell, and has more recently in Germany been illuminated by the example and inspiration of Helmholtz. Care is taken to avoid an irrevocable formulation of the problem in advance, only its broad dynamical features being worked in; while all the light that cognate but better understood branches of physics can shed by way of illustration or analogy is pressed into service. Thus, instead of writing out isolated equations of motion for the ideal case of a single electron—on the tacit assumption that no other electrons are near which would disturb the averaged field that is alone supposed to affect it—it is recognised that electrons, possibly in very large number, are somehow involved in the structure of each individual molecule, and that the fundamental and essential element in the physics of matter in bulk is this permanent molecule considered as a single free vibrating system, with free periods producing a radiant spectrum, which are involved in the intrinsic mutual influence of these oscillating electrons. The simplest type of framework for the structure of such a system is to assume provisionally a gyrostatic orbital constitution of some kind, which assists in holding its parts together in some such way as the whirling motion holds together a vortex ring in fluid. Our dynamical plan is thus now no longer fixed, but flexible; in fact it must remain so until we can form a definite representation of such a molecule instead of only a general idea of it. Yet the uniformity of physical law for matter in bulk shows that we ought to be able to develop our synthesis without waiting for such knowledge, which may even quite possibly be unattainable. In this procedure we must attend primarily to such activities of the molecules as can be cumulated by addition, so as to produce aggregate results expressible per unit volume of the medium, and eliminate the remaining non-cumulative disturbance which is related to practically irreversible or thermal phenomena. Of the former class is the strain in the configuration of the molecule produced by the electric or magnetic field in which it is situated, this distortion being represented for statical purposes by a single vector quantity, the induced electric or magnetic moment of the molecule, which aggregates into induced polarity of the material medium. Such, also, are the types and energies of free vibration about the steady configuration, which have been analysed in their aggregate into definite periods by the spectroscope. Here our knowledge is related to general principles rather than special systems, and progress is possible, thanks to the purely abstract general formulation of dynamics by Lagrange and Hamilton, and also to the supports and signposts afforded by such phenomena as anomalous optical dispersion and the

magnetic subdivision of spectral lines. Outside such properties our power of tracing relations is very limited and imperfect, in the absence of control over the individual molecules, there being little to go upon except the two principles of thermodynamics, those of energy and entropy. The difference between the two points of view, the definite but partial and limited mathematical illustration and the wider but largely undetermined model, crops up, for example, in M. Poincaré's discussion of conducting media (§417), in which the combination of polarisation with conduction arising from wandering ions does not appear to suggest itself: "remarquons d'abord qu' ayant affaire à des conducteurs on n'a plus de polarisation," so that he has only to deal with electric current and electric density, doubtless in some degree with a view to save complication in a didactic exposition; whereas from the other more physical representation of a material medium one does not readily conceive a state of affairs in which only conduction and convection of free charges are present, but would proceed rather to examine under what circumstances polarisation can be practically neglected in comparison with conduction. This, of course, can be done in ordinary electrodynamics, as was doubtless in the writer's mind. But in the optical phenomena of metals it was recognised by Maxwell himself from the earliest stages—thanks mainly to the physical models on which he cultivated his ideas—that both agencies were essential; while recent closer examination has shown (cf. *Phil. Trans.*, 1895 A, p. 711, and in detail in recent papers by Drude) how naturally their combination represents the general features of metallic reflection as revealed by the most valuable and extensive measurements of Drude and other experimenters.

In working out the analysis, our author follows Lorentz in calculating directly the electrodynamic effects propagated from the moving electric charges which are the source of all the disturbance. He expresses this in terms of the "retarded vector potential" of the true current, a vector whose components are the potentials of the three components of its distribution, considered, however, as travelling out from them and becoming established around them with the velocity of radiation. A procedure which concentrates attention on the simply extended though molecularly constituted medium, to the exclusion, as far as possible, of the individual moving electrons, can get on more simply in Maxwell's manner by using the fictitious total current, which includes æthereal displacement as well as translation of charges; then the retardation of the vector potential is dispensed with, and all the functions are referred to the same instant of time, so that attention can be concentrated on the processes of averaging, undisturbed by mathematical complexities.

The distinction above sketched between the crystallised mathematical and the fluent physical point of view is at the root of what is a prominent characteristic of the writer's criticism. The development of electrodynamics appears as split up into so many independent and largely irreconcilable theories; there are headings, "theorie de Weber, de Maxwell, de Helmholtz, de Hertz, de Lorentz, de Larmor." Whereas on the view which works by models and general ideas rather than by formulas there is but one theory of electrodynamics—at any rate only one æther-theory—which has put on various modifications and has adopted various forms of expression, in the course of gradual improvement so as to become a closer and closer mental picture of the orderly course of phenomena; the subject presents itself rather as a continuous historical development, into which somewhat different paths all converge, than as a series of competing modes of explanation.

There is one feature in M. Poincaré's exposition for which the English reader will be grateful. A considerable trouble in the assimilation of mathematical investigations on this subject is the diversity of the notations (not to speak of systems of units) that are in use. All the available letters of most available alphabets have been pressed into service to represent the numerous types of quantities that occur; and if there is not a consistent basis of usage it must follow that the same symbol will be made to represent different things by different writers. M. Poincaré has kept as close as circumstances allowed to Maxwell's own notation, thereby acting up to the appeal of Boltzmann ("Vorlesungen über Maxwell's Theorie," 1891, preface), who found it necessary to actually construct a key for his own use to connect the notations of the principal German writers. Although the simplifications introduced by Heaviside, and subsequently in more formal guise by Hertz, did much to clear away the unessential accumulations that had overlaid Maxwell's theory, they did not in any sense transform it; and recent developments may be held to have justified the superiority, as a working basis for further advance, of the original elastic framework in which Maxwell set the theory, over the condensed *précis* of established results by which Hertz temporarily replaced it. It seems, therefore, unfortunate that the condensation of notation which was a part of Hertz's modification should have reacted to introduce some confusion in the notation of the more complete theory.

The development of electrodynamics, which was firmly established as the proximate foundation of all physical science, certainly of all that has relation to the æther, by Maxwell about forty years ago, has been going on with rapidly accelerated progress, both on the experimenta and on the theoretical side, during the last ten years. New points of view have rapidly come up, have sometimes been as rapidly transcended. It is not surprising, therefore, that the discussion in the last chapter, which mainly relates to the mechanical and *quasi*-mechanica models of the British school, is somewhat out of date, indeed, it is largely constructed on the basis of an abstract, published in advance, of an imperfect first draft of theory contained in a memoir of date 1894, much as a palæontologist reconstructs a fossil organism from some of its bones. In the recent Lorentz-memorial volume M. Poincaré has himself revised some of his positions.

It is by this sort of discussion that crude theories are sifted and worn down into symmetry and order. And it is matter of congratulation that an analyst of M. Poincaré's vast command of all the resources of modern mathematics finds time not only to apply his genius to a thorough revision of the methods of mathematical astronomy, but also to survey the field of general physics as he has done in this interesting volume. In these days of extreme specialisation such surveys promise a special harvest, but few have sufficient breadth of learning to

undertake them. The modern development of the theory of functions arose largely from transplanting the ideas of flux and force of physical mathematics into purely abstract problems. In astronomy, M. Poincaré's work has partly repaid the debt; it remains to be seen whether in electrodynamics a further instalment will be repaid, or analysis again become the debtor.

Anyhow, while pure analysis is ramifying into vast new regions and becoming more and more specialised, it is fortunately still possible for a single person to acquire an effective knowledge of the whole domain of theoretical physics. As in literature, so in scientific exposition, the saving virtue is style. If we call to mind the history of any of the theories which form the established heritage of common knowledge—such as hydrostatics or pneumatics—in their early inception they presented just as complex problems as the theory of the æther does now. But by the efforts of successive generations of expositors they have gradually been worn down, and the artificial appliances of symbolic reasoning have been eliminated or illuminated by the cultivation of new ideas and modes of expression. A theory of the æther hardly existed in any adequate sense half a century ago. Progress has recently been so rapid both on the purely scientific side, and in the reaction of modes of thought that have been fostered by industrial developments, that in a short time we may be able to picture to ourselves the operation of the æther with as much clearness and directness as we now understand the functions of the atmosphere.

J. L.

GILBERT WHITE OF SELBORNE.

The Life and Letters of Gilbert White of Selborne. Written and Edited by his Great-Grandnephew, Rashleigh Holt-White. Two Vols. 8vo. Pp., Vol. I., xv + 330; Vol. II., ix + 300. (London: John Murray, 1901.) Price 32*s.*

The Natural History of Selborne. By Gilbert White. Pp. vii + 381. (London: J. M. Dent and Co.) Price 1*s.* 6*d.* net.

NOTHING nearly as good as Mr. Holt-White's book has ever yet appeared about Gilbert White; it supersedes Bell's two volumes, to which we have so far had to go for the real characteristics of the great naturalist, and it is hardly possible that it will ever itself be superseded. In its skilful treatment of materials it is amply worthy of its dedication to a great scholar, the present Provost of White's College. The editor has been content to let White and his correspondents speak for themselves, but rarely interposing to set us right on some misconception, or to explain (often, it is clear, after much expenditure of time and trouble) who are the persons referred to in the correspondence; and the result is one of the most delightful stories of a quiet life ever told in our language. As we reluctantly close the second volume, we feel that we now know White perfectly well as he really was. There is no need for a reviewer to anticipate the pleasure of readers by attempting to copy the picture.

It should be said, however, that this is not only a book for naturalists or lovers of nature, but for readers of every kind. Indeed, the charm of it seems to lie chiefly in the picture of life and manners it gives us—of the life of quiet country folks, with sedate but real interests of their own, using their time well, and sharpening their faculties continually under the gentle and unconscious stimulus of their alert and keen-eyed neighbour, friend or uncle. Gilbert White is the centre of the group, and he seems to be setting all the members of it at work on something. He lets drop a hint, asks a question, administers a very gentle reproof, and the recipients of his letters treasure them up, and must, we feel, have acted on them.

One or two points of special interest may be noted here. It is very pleasant to find that Mr. Holt-White has been able to prove conclusively the falsity of the traditional Oriel notion that White retained his fellowship when he should not have done so. The four or five farms which he inherited brought him hardly more than a hundred a year; and towards the end of his life his relations with his College seem to have been quite cordial. It is, of course, natural that in a College where Fellowships were few in number, yet open to competition from the whole University, the locking up of a Fellowship for fifty years should at the time have roused a certain amount of criticism; but that criticism was made under the impression that White was a wealthy man, and to revive it, as it has been revived, in these days, is to do White a serious injustice. The Oriel of that day may be said to have endowed science unconsciously as it has never done since; for White, though not a man of science in the modern sense, has had a powerful influence in stimulating scientific habits.

Among the many delightful treasures in this book must be mentioned the letters of Thomas Mulso, now published for the first time—letters as bright, witty and natural as any that have ever been printed; and the two letters of Montagu, written after the publication of the "Natural History of Selborne," which offer a curious contrast, in their intense and almost feverish ardour, to White's quiet and leisurely way of going about his work. But perhaps those who love the eighteenth century and all its ways will find their greatest pleasure in the enthusiastic diary of Miss Kitty Battie, a visitor at Selborne. Little did that happy girl know that her notes, jotted down in the fulness of a grateful heart, would be treasured more than a century afterwards by readers as enthusiastic as herself.

Let us hope that this work, undertaken by a member of the White family, with full access to all records, and with the invaluable aid of Prof. Alfred Newton, may permanently satisfy all who wish to know about White's character and habits.

The second book mentioned at the head of this notice is a handy little volume in small octavo, which can be carried in the pocket, and has the great merit of being free from unnecessary notes and still more unnecessary illustrations. The few notes which it contains, by Mr. Charles Weekes, are at the end of the volume, and seem to be for the most part accurate and to the point. The text is reprinted from the first edition of the "Natural History," with a few slight alterations in spelling, which might perhaps have been dispensed with. If, for example, White wrote "plowed," there is no reason at all why an editor should substitute "ploughed." And it is a pity that the editor, in prefixing a few lines of Richard Jefferies' to the book, should not have spelt his name correctly. But on the whole the edition is a good one; far better, in fact, than many of much greater pretension.